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Work–Family Policy Trade-Offs for Mothers? Unpacking the Cross-National Variation in Motherhood Earnings Penalties

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Abstract

Recent scholarship suggests welfare state interventions, as measured by policy indices, create gendered trade-offs wherein reduced work–family conflict corresponds to greater gender wage inequality. The authors reconsider these trade-offs by unpacking these indices and examining specific policy relationships with motherhood-based wage inequality to consider how different policies have different effects. Using original policy data and Luxembourg Income Study microdata, multilevel models across 22 countries examine the relationships among country-level family policies, tax policies, and the motherhood wage penalty. The authors find policies that maintain maternal labor market attachment through moderate-length leaves, publicly funded childcare, lower marginal tax rates on second earners, and paternity leave are correlated with smaller motherhood wage penalties.

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Gendered economic inequality persists among welfare states, despite a wide range of work–family policies ostensibly aimed at addressing these inequalities (Budig & England, 2001; Charles, 2011; Charles & Grusky, 2004; Gornick & Meyers, 2003; Mandel & Semyonov, 2005, 2006; Pettit & Hook, 2009; Yaish & Stier, 2009). Previous research suggests that these policies may either ameliorate or exacerbate the degree of gender economic inequality (Abendroth, Huffman, & Treas, 2014; Korpi, Ferrarini, & Englund, 2009; Mandel, 2009; Mandel & Semyonov, 2005, 2006; Orloff, 1993; Pettit & Hook, 2005, 2009). Most scholars now view work–family policies as not either entirely good or bad, but leading to *trade-offs*, which reflect diversity in outcomes (education, labor force participation, occupation, wages, poverty, etc.) and in women's experiences—by parenthood, class, and other characteristics. For example, transfers for caregivers may reduce gender inequality in the risk of poverty while increasing gender inequality in wages by lowering mothers' accumulated experience. To understand whether and how work–family policies create trade-offs for mothers' earnings, we compare the earnings of women with varying numbers of children to show the relationship between the wage penalty for motherhood and specific work–family policies.

While some researchers see state interventions as increasing women's employment and wages (Gornick & Meyers, 2003), others suggest that these interventions lead to trade-offs for women in the form of lower employment or greater gender pay gaps (Mandel, 2009; Mandel & Semyonov, 2005, 2006; Pettit & Hook, 2009). Many of the work–family policies considered important to explaining gender inequalities specifically target *motherhood*. There is growing heterogeneity among women with respect to parenthood and labor market behaviors. Research on the impact of children on women's earnings, or the *motherhood penalty*, shows significant economic inequality among women related to motherhood and the number of children (Abendroth et al., 2014; Budig & England, 2001; Budig & Hodges, 2010; Gangl & Ziefle, 2009; Harkness & Waldfogel, 2003; Joshi, Paci, & Waldfogel, 1999; Waldfogel, 1997, 1998a, 1998b). This body of research defines the motherhood penalty as the amount each additional child lowers women's earnings.

Judging the effectiveness of state interventions, then, means that we must understand not simply whether they reduce gender inequality but whether they reduce inequality *among women with varying numbers of children*. Thus, focusing on gender gaps between the *typical* woman and the *average* man misses the variation among women in regard to the impact of specific work–family interventions (maternity leave, public childcare, etc.) on labor force participation and earnings. To more clearly understand how state interventions shape the impact of women's responsibilities for children on their labor market outcomes, we focus our analysis *on women* to examine the association of work–family policies with the size of the motherhood earnings penalty (the effect of each additional child on a woman's earnings, controlling for other factors) in 22 countries. In this way, we move the literature beyond a broad comparison of gender differences to a sharper focus on state interventions, motherhood, and market work.

Our article brings together two major literatures. One is focused on the motherhood penalty or the negative effect of each additional child on women's earnings. The second is focused on gendered welfare state outcomes, and in particular, the complex, and perhaps contradictory effects of welfare state policies on outcomes for women and mothers. We bridge these literatures to more fully explore the complex intersections of state, family, and markets in contemporary welfare states (O'Connor, Orloff, & Shaver, 1999). Our central contributions are threefold: First, we focus on the earnings consequences of motherhood, rather than gender, across 22 countries to understand how country-level work–family policies specifically shape the motherhood earnings penalty. Second, we break with the comparative *regime* approach to examine how particular work–family policies are related to the size of motherhood penalties in varying country sociopolitical contexts. Third, we unpack the welfare state trade-offs debate by disaggregating work–family policies to identify policies that are associated with greater or smaller motherhood wage penalties. Our aim is to develop an integrated, and more nuanced, understanding of these relationships among families, markets, and states.

Earnings Penalties for Motherhood

It is well established that children are linked to reduced earnings for women in most wealthy countries (Abendroth et al., 2014; Anderson, Binder, & Krause, 2003; Avellar & Smock, 2003; Budig & England, 2001; Budig & Hodges, 2010; Davies & Pierre, 2005; Gangl & Ziefle, 2009; Glauber, 2007; Harkness & Waldfogel, 2003; Joshi & Newell, 1989; Joshi et al., 1999; Lundberg & Rose, 2000; Waldfogel, 1997, 1998a, 1998b). Previous cross-national work suggests substantial variations in the size of these penalties (Davies & Pierre, 2005; Gangl & Ziefle, 2009; Harkness & Waldfogel, 2003), although we know less about the impacts of welfare state policies on the size of these penalties.

Analytically, examining how these processes differ cross-nationally, while controlling for individual-level differences, allows us to theorize how societal context matters for labor market outcomes (Fernandez-Macias, 2012; Yaish & Stier, 2009), such as the motherhood penalty. A wide range of scholarship explores labor market processes and inequality cross-nationally (DiPrete, 2005; Ebbinghaus & Kittel, 2005; Fernandez-Macias, 2012; Gangl, 2005; Hancke & Rhodes, 2005; Maurin & Postel-Vinay, 2005) to suggest that meaningful differences can be attributed to variations in labor market institutions. Other scholarship examines how differing societal contexts shape the work–family nexus, including wage effects (Abendroth et al., 2014; Charles & Grusky, 2004; Edlund, 2007; Gornick & Meyers, 2003; Mandel & Semyonov, 2005, 2006; Pettit & Hook, 2009; Ruppanner & Huffman, 2014).

The motherhood wage penalty estimates how much less women earn for each additional child they have. The individual-level mechanisms that produce this penalty have been extensively studied (Anderson et al., 2003; Avellar & Smock, 2003; Budig & England, 2001; Budig & Hodges, 2010; Gangl & Ziefle, 2009; Glauber, 2007; Lundberg & Rose, 2000; Waldfogel, 1997, 1998a, 1998b). These studies show that the total, or gross, motherhood penalty can be partially explained by foregone work experience due to childbirth interruptions, firm changes following employment reentrance, and part-time work hours, among others.[1] Yet, contextual factors, including work–family and other policies, mitigate the extent to which motherhood impacts women's employment. Both individual- and country-level factors shape the motherhood penalty. We detail these multilevel pathways in Figure 1. In this figure, pathways between factors empirically proven to affect the motherhood penalty are shown as solid lines. Dashed lines represent the pathways we investigate in the current study. Through a series of nested multilevel regression models, we show how individual differences among women (Level 1) partially account for the motherhood earnings penalty, as measured by number of children in the home. At Level 2, we examine the relationship between the per-child motherhood earnings penalty (net of individual and household characteristics) and measures that capture country-level policies salient for employed mothers.

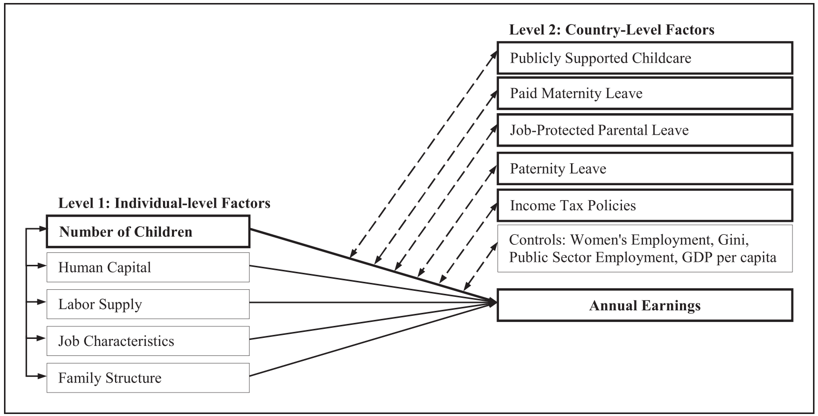

Figure 1. Factors impacting the earnings penalty for motherhood, two-level model.
GDP = gross domestic product.

Public Policies and Motherhood Earnings Penalties

A range of societal-level factors may account for the negative impact of children on women's earnings. We focus on those interventions that address how families and markets intersect. For example, work–family policies include maternity and parental leave and subsidized or state-provided childcare—policies meant to help ensure that parents can balance care and employment. Taxation policies may either reward or penalize dual-earner couples, further affecting how couples make choices regarding employment, although tax effects may equally apply for childless couples. We explore whether these policies might be associated with variations in earnings penalties cross-nationally.

Welfare state scholarship has explored whether policies aimed at women's reconciling employment with care—such as leaves and childcare—have perhaps unintentionally disadvantaged women, or groups of women, or led to trade-offs (Albrecht, Bjorklund, & Vroman, 2003; Charles & Grusky, 2004; Glass & Fodor, 2011; Mandel, 2009; Mandel & Semyonov, 2005, 2006; Pettit & Hook, 2009; Yaish & Stier, 2009). Counter to expectations that work–family policies would support women's employment and ameliorate inequality (Gash, 2009; Gornick & Meyers, 2003), these policies may, themselves, undermine women's—and particularly mothers'—employment. Much of this literature focuses on broad gender gaps between men and women, rather than looking more particularly at differences among women and mothers. Yet, whether these policies are *friend or foe* to mothers may depend on the outcomes measured (Bianchi & Milkie, 2010; Mandel, 2009). Thus, paid parental leaves may maintain family finances while also weakening mothers' employment—serving as both *friend and foe*. If we focus on one particular outcome, such as mothers' employment, different policies may have different effects. For example, publicly funded childcare may support mothers' employment, reducing employment differences among women. In contrast, long parental leaves may weaken mothers' attachment to the labor force, increasing the motherhood penalty through foregone experience (Boeckmann, Misra, & Budig, 2015).

Many scholars contend that work–family policies increase women's employment and wages, by helping them balance the demands of both family and work, and make this argument by comparing broad welfare state regimes (Daly, 2000; Esping-Andersen, 1999; Gornick & Meyers, 2003; Korpi, 2000; Orloff, 2002). A limitation of this comparative case approach is that it is difficult to disentangle policy effects with other country-level differences in, for example, culture or broader earnings inequality. Moreover, the regime framework mutes within-regime variation on these dimensions. To better model individual outcomes and country-level effects, researchers have used multilevel modeling strategies with larger samples of countries to examine gendered policy outcomes (Boeckmann et al., 2015; Mandel & Semyonov, 2005, 2006).

By using an index of work–family policies that include public sector employment, leaves, and childcare, Mandel and Semyonov (2005, 2006) argue that positive outcomes are not guaranteed; instead, there are important trade-offs worth considering. But a limitation of this policy index approach is the diversity of the work–family policies summarized in such indices.[2] Some policies, such as extended parental leaves, may have markedly different effects on maternal employment and earnings than other policies, such as high-quality publicly subsidized childcare. Despite this diversity, scholars often have subsumed an array of policies into an overall index to assess their impact on employment and earnings (Gornick, Meyers, & Ross, 1997; Mandel & Semyonov, 2005). We adopt a multilevel approach modeling these policies separately because we believe that they may reflect different gendered assumptions about women's and mothers' roles.

What policies and other contextual factors may influence the motherhood penalty? We identify at least three factors that may influence mothers' abilities to combine work and care, and therein impact their ability to maintain employment and job experience accrual: (a) the prevalence of publicly funded childcare, (b) the duration and benefits levels of maternity and parental leave policies, and (c) the effect of taxation policies on the net pay of the second earner (Evans, 2002; Gauthier & Bortnik, 2001; Gornick & Meyers, 2003; Jaumotte, 2003a; Morgan & Zippel, 2003; Pettit & Hook, 2005, 2009).

We examine these policies separately because different gendered assumptions may underlie them; for example, extended parental care policies may be undergirded by an assumption that mothers should directly care for children at home, while publicly funded childcare policy may reflect an assumption that women should be able to pursue paid employment when children are young. Korpi et al. (2009) similarly note that within countries, there may be "competing values and conflicting goals concerning relationships between women, men, and families" (p. 3). Because policies are the results of historical processes in which multiple actors and societal groups may have a say, different policies within countries may embrace different values or goals (Morgan, 2005; Morgan & Zippel, 2003). For these reasons, it is important to consider different state interventions independently. A generalized index that combines policies that alternately promote or deter paid employment among new mothers may therefore obscure policy effects on their employment outcomes (Korpi et al., 2009). By analyzing policies separately and focusing on differences among women, we consider the effects of different state interventions on earnings in a more nuanced way. We also consider policy combinations by investigating whether tax policies regarding the second earner's income have stronger or weaker effects once we account for childcare and parental leave policies.

Childcare policies. Childcare policies might impact cross-national differences in mothers' earnings through enabling more continuous employment when children are very young (McDonald, 2000). While childcare programs were adopted both to educate children and to support parents' employment, programs for children under 3 are explicitly recognized as helping families balance care and employment (Gornick & Meyers, 2003; Kamerman & Kahn, 1991). In addition, childcare costs are strongly correlated with women's employment. Han and Waldfogel (2002) argue that in the United States, reducing childcare costs to parents could substantially raise employment of both married and single mothers. Because government funding and subsidies tend to reduce the cost of childcare to parents while keeping the quality of care high (Organization for Economic Co-Operation and Development [OECD], 2001), we focus on publicly supported, rather than market-based, childcare. Cross-nationally, Pettit and Hook (2005, 2009) show that high levels of childcare are positively linked with women's labor market participation, while Abendroth et al. (2014) show an association between lower penalties and investment in public childcare. This leads us to predict Hypothesis 1:

> Hypothesis 1: *The proportion of children enrolled in government-provided or -subsidized childcare should be negatively related to the earnings penalty* by allowing mothers to remain more continuously in paid employment, and therein minimize lost job experience. We use separate measures for policies that apply to infants (<age 3) and those that apply to preschoolers (ages 3 to 6).

Leave policies. Leave policies (i.e., maternity, paternity, and parental leave)[3] are meant to support temporary caregiving while allowing parents to return to the same or equivalent job. We consider both benefit levels of leaves and duration of leave. Benefit levels matter because very low-paid leaves may be less effective than well-paid leaves, with families unable to take advantage of the leave. The duration of leave may also have varying associations with the motherhood penalty. For example, very long parental leaves could decrease mothers' employment continuity and increase lost job experience and penalty for motherhood (Morgan & Zippel, 2003; Pettit & Hook, 2005, 2009) by reducing labor force attachment. Moreover, the prospect of mothers' prolonged absence from work might discourage employers from hiring or promoting young women and mothers (Glass & Fodor, 2011). Yet, the absence of statutory leave entitlements may also increase the motherhood penalty by forcing women to exit the workforce during the child's early years of life, and therein reducing job experience and making mothers less attractive to employers as long-term workers. In contrast, moderate job-protected leaves may help mothers maintain labor force attachment and encourage timely returns to employment, thus minimizing productivity costs to employers and mitigating lost job experience related to maternity. Indeed, studies show curvilinear effects of leave length on women's employment outcomes and poverty (Boeckmann et al., 2015; Evertsson & Duvander, 2010; Kenworthy, 2008; Pettit & Hook, 2005, 2009).

> Hypothesis 2a: Paid leaves should be negatively associated with motherhood penalties.

We measure paid leaves as (a) number of fully paid weeks of maternity leave and (b) number of fully paid weeks of parental leave.

> Hypothesis 2b: The duration of parental care leaves should matter for lost job experience and employer tenure and will not be captured in the calculated number of weeks of fully paid parental care leaves above. Thus, we predict that the duration of care leave, regardless of benefit level, should have curvilinear associations with the motherhood penalty.

No or very short leaves will be linked to higher motherhood penalties. Moderate leaves should decrease the motherhood penalty. In contrast, very long leaves (e.g., 2 to 3 years) should increase the motherhood penalty. We measure duration of women's leaves in terms of the maximum number of job-protected weeks of leave available to women (regardless of availability and level of benefits), including a squared term for leave length to model curvilinear effects. We also test for cubed and higher order transformations of leave length to detect and model curvilinear effects.

Taxation policies. Income taxation policies influence the amount of disposable income available to families and may shape (married) women's decisions about employment (Sainsbury, 1999). Notably, in many countries, second earners' incomes are taxed more heavily than single earners (Jaumotte, 2003b for 2000/2001), which may provide a disincentive to women to take up (full-time) employment. Given the complexity of tax systems, in which the tax burden may depend on multiple factors,[4] the body of literature examining the relationship between income tax policies and women's employment participation has not lead to conclusive results (Sainsbury, 1999; van der Lippe & van Dijk, 2002). However, studies show that tax disadvantages to second earners tend to be related to lower female employment participation. For example, Jaumotte (2003a) finds that in a sample of 17 OECD countries, higher ratios between the tax rates of a second earner in a coupled household and a single earner (who both earn 67% of the average production worker's earnings) are inversely related to women's employment rates. Sainsbury (1999) concludes that tax policies help explain lower female employment participation in European welfare states where the tax systems imposed considerable penalties on working wives' incomes. Here, we examine tax disincentives to partnered women's employment, who make up the majority of our sample. By influencing women's labor market attachment, tax policies may shape the earnings penalties connected with motherhood.

> Hypothesis 3: Taxation policies that penalize second earner's incomes in coupled households should be positively correlated with higher motherhood earnings penalties by discouraging (partnered) women's labor market participation when children are small resulting in interrupted attachment to the labor market and reduced experience due to childbearing.

Yet, we consider this a fairly conservative test, given that taxation policies may also lower partnered childless women's labor market participation, resulting in part-time work or reduced experience.

In addition to considering how specific policies are related to the motherhood penalty, we also examine the resilience of policies aimed at supporting women's capacity to care for children and remain employed (job-protected leaves and publicly funded childcare) when we include tax rate policies that make the second earner's wages more or less valuable to the family economy.

We use a multilevel modeling strategy to control for the individual-level factors known to partially explain the motherhood earnings penalty and simultaneously estimate how country-level factors alter net penalties for children. Our modeling strategy allows us to consider how policies meant to mediate family responsibilities are associated with the wages of women with differing responsibilities for care. Our goal is to assess the effects of state interventions on inequalities linked to gendered caregiving. By unpacking welfare state policy indices into specific policy measures, we expect our work to adjudicate debates in the welfare state literature regarding whether such interventions create a paradox (Mandel & Semyonov, 2005, 2006) or trade-off (Mandel, 2009; Pettit & Hook, 2009) while also addressing assumptions in the motherhood penalty literature regarding how contexts may explain variations in motherhood penalties. As a result, we specify how state interventions in the intersection of families and markets matter to wage inequality among women.

Controls for Individual-Level Factors on Earnings Penalties for Motherhood

While we focus on how work–family policies condition the relationship between family and the market for women, we first ensure that we are controlling for factors that may partially explain the motherhood earnings penalty at the individual level. This is because these individual factors—such as mothers' educational attainment—may differ from country to country, while our aim is to capture the policy effects rather than such differences across populations.

A large body of research has established the impact of children on women's earnings and the individual-level factors that shape this relationship (Anderson et al., 2003; Avellar & Smock, 2003; Budig & England, 2001; Budig & Hodges, 2010; Lundberg & Rose, 2000; Sigle-Rushton & Waldfogel, 2004; Waldfogel, 1998a, 1998b). First, family structure and household resources affect the motherhood penalty. In the United States, married women incur larger penalties for motherhood in the United States (Budig & England, 2001; Budig & Hodges, 2010; Glauber, 2007), while gross motherhood penalties are larger for single women in some countries, while in still others, there is no difference between single and married or partnered[5] mothers (Gangl & Ziefle, 2009; Killewald & Gough, 2013). In addition to partnered status, other household income, including partners' earnings and transfer income from the state or private sources, may impact women's decisions to engage in paid labor, and therein affect the motherhood penalty.

Human capital and work effort (measured by labor supply) profoundly shape the motherhood penalty. Smaller or no penalties are found among the highly educated, both in the United States (Amuedo-Dorantes & Kimmel, 2005; Anderson et al., 2003; Taniguchi, 1999) and cross-nationally (Todd, 2001). In addition, mothers' lower labor supply, measured as hours worked or part-time status, explains an additional portion of the penalty for children (Budig & England, 2001; Waldfogel, 1997), but a significant penalty remains even after controls for human capital and labor supply are added.

Lost job experience due to breaks for childbearing is one of the major mechanisms negatively affecting mothers' earnings trajectories. Women with (more) children typically have less experience and seniority due to the employment breaks taken to accommodate childrearing, and this explains one third to over one half of the motherhood earnings penalty (Budig & England, 2001; Klerman & Liebowitz, 1999; Staff & Mortimer, 2012). Lost experience explains more of the penalty (50%) among highly paid skilled workers where returns to experience are stronger (Budig & Hodges, 2010). While the microdata used in this study do not include a consistent measure of job experience for the 22 countries we analyze, even if possible, such a measure would be endogenous to the model. Many of the state interventions that are associated with the size of the motherhood penalty operate through their impact on the amount of job experience women lose following childbirth or adoption.

Furthermore, differences in the motherhood earnings penalty across countries could be due to differential selection of mothers into employment across countries. Mothers' employment rates vary dramatically from country to country making it particularly important that we consider potential selection bias (Boeckmann et al., 2015). To address this differential selection, we use a two-stage Heckman selection correction approach, which we discuss later (Heckman, 1979).

In addition to human capital and labor supply, the motherhood penalty may be shaped by compensating differentials (Gash, 2009). To the extent that mothers trade earnings for jobs that have more family-friendly characteristics, these characteristics may partially explain the motherhood penalty. While one study (Budig & England, 2001) found no effect of job characteristics on the penalty in the United States, other work shows the penalty is larger among women in nonprofessional or nonmanagerial occupations (Budig, 2006). Some scholars argue that female-dominated occupations are potentially more family-friendly (Gangl & Ziefle, 2009). To examine whether the motherhood penalty can be explained by a differential distribution of childless women and mothers across professional-managerial occupations, or across gender segregated occupations, we include controls for these factors in some models. However, these measures are likely to be endogenous to the model (predicted by number of children a woman has) and are not included in all estimations.

Yet, even in models that include all of the individual-level factors discussed earlier, a significant penalty persists in many countries in Europe and North America (Budig & England, 2001; Budig & Hodges, 2010; Budig et al., 2012; Gangl & Ziefle, 2009; Harkness & Waldfogel, 2003). Possible explanations for this unexplained penalty among mothers include employer discrimination, lowered productivity, or in some contexts, inadequate affordable childcare options and the absence of paid family leave. Correll, Benard, and Paik (2007) provide evidence of employer discrimination with their experimental research in the United States, as do Glass and Fodor (2011) with their research based on interviews of employers and antidiscrimination cases in Hungary. While our design does not measure employer discrimination, it does allow us to consider how state interventions may be associated with the motherhood earnings penalty despite diverse socio-political-economic contexts. In Figure 2, we show the variation in the size of the per-child motherhood penalties across the countries we analyze, estimated using Luxembourg Income Study (LIS) data.

Figure 2 shows the exponentiated coefficient of the number of children from country-by-country Heckman selection models including covariates for education, age, part-time status, and partnered status.[6] Solid bars show significant motherhood coefficients, while empty bars show nonsignificant coefficients. This graph demonstrates that the per-child motherhood penalties vary across countries in our analysis. While we do not find a significant impact of the number of children on women's earnings in Russia, Australia, and Israel, significant per-child motherhood penalties net of controls remain in a majority of the countries (with the largest penalties found in continental European countries, such as West Germany and the Netherlands, and smaller ones in a number of postsocialist Eastern European countries, but also France

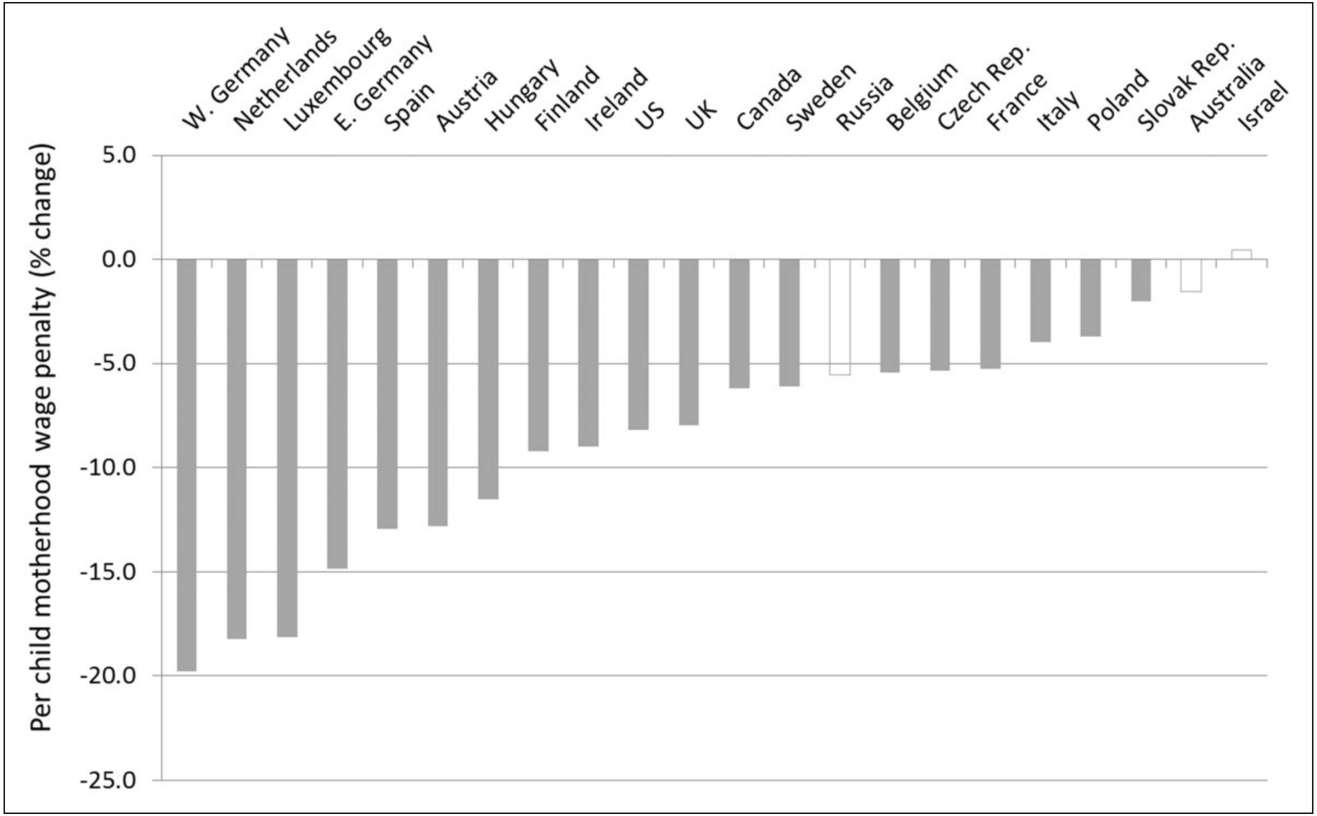

Figure 2. Adjusted per-child motherhood wage penalties (from Heckman selection models controlling for human capital labor supply and family structure).

and even Italy). This suggests that country-level differences, such as work–family policy configurations, may matter for how large an earnings penalty mothers incur, as our analysis will investigate.

Data and Measures

Our study uses data from multiple sources. Individual-level data come from the Cross-National Data Center in Luxembourg (LIS). The LIS is an excellent source of secondary survey data on households, employment, and earnings and provides the best cross-national microdata for comparing income across OECD countries (OECD, 1995). With a few exceptions, we use Wave 5 (representing the years 2000/2001)[7] of the LIS data for 22 countries. For all countries, the sample is restricted to employed women, aged 25 to 45 (prime years for childrearing), who are not self-employed and are not in the military. The total individual-level sample size is 68,254 women with country samples varying between 545 women in the Austrian data set to 13,544 in the data from the United States. The median sample size is 1,523. We present sample sizes, the weighted means, and standard deviations for our individual measures (for both mothers and childless women) in each country in Table A1 of the Appendix.

Our dependent variable is the natural log of annual earnings in 2,000 U.S. constant dollars.[8] While much of the motherhood penalty research uses hourly wage as the dependent variable, this measure is not available broadly or consistently in the LIS data. Clearly, both differences in hourly wage rates and hours worked will generate differences in annual earnings. To capture the impact of mothers' greater likelihoods of working reduced schedules, our models include a measure for part-time hours. A quantitative measure of work hours is not consistently available across all countries. Thus, if among full-time workers, women with fewer children work more excessive overtime hours, the motherhood penalty in annual wages may still partially result from unmeasured differences in work hours among the full-time workers.

Consistent with past research on the motherhood penalty, our primary independent variable captures the number of dependent children coresiding with the respondent (Anderson et al., 2003; Budig & England, 2001; Budig & Hodges, 2010; Gangl & Ziefle, 2009; Staff & Mortimer, 2012; Taniguchi, 1999; Waldfogel, 1997). One limitation of LIS data is that it does not include detailed measures of motherhood, and we cannot draw upon fertility histories. This likely leads to underestimation of the per-child penalty because our measure of motherhood does not capture the effect of nonresidential offspring on wages. We also tested alternative specifications for motherhood include a dummy variable for mother, or separate dummy measures for one-, two-, and three-plus children. Results were generally robust (results available upon request). Following general practice in the literature, we present the findings with our measure of number of children because it is more easily interpreted than multiple dummies, which would each require multiple cross-level interactions.[9] The effect of each additional child is monotonic, though not always perfectly linear in all countries.

Individual-level independent variables include family composition, human capital and labor supply, and job characteristics. Family characteristics include, in addition to number of children, relationship status (married or cohabiting = 1, otherwise = 0). Human capital measures include educational attainment measured with a dummy variable = 1 to indicate tertiary education or specialized vocational education leading to licensing or other credentials comparable with a college degree.[10] We use respondent's age as a limited proxy for labor market experience.[11] We include a dummy for part-time work, defined as those working less than 30 hours weekly.[12] Some models include job characteristics; these measures include a dummy variable indicating that the respondent holds a professional or managerial occupation and the percentage female of the occupation, derived from aggregated LIS data.[13] We include these measures because past research suggests that mothers may trade earnings for jobs with family-friendly characteristics (supportive female-dominated jobs or less stressful nonprofessional work; Budig & England, 2001).

For policy indicators, we use leave and childcare measure from the work–family policy indicators database (Boeckmann, Budig, & Misra, 2012); taxation policy data come from Florence Jaumotte's (2003b) database. The work–family policy indicators database is modeled after those developed by Gornick and Meyers (2003), Gornick et al. (1997), and Gauthier and Bortnik (2001). Our database includes 22 countries: Australia, Austria, Belgium, Canada, Czech Republic, Finland, France, East Germany, West Germany,[14] Hungary, Ireland, Israel, Italy, Luxembourg, Netherlands, Poland, Russia, the Slovak Republic, Spain, Sweden, the United Kingdom, and the United States. We match our policy measures to the LIS survey year for each country, lagging the measurement of leave policies to 2 years prior to the survey year.[15]

We present policy measures for each country in Table A2 of the Appendix. Childcare policy includes the percentage of children aged 0 to 2 and the percentage of children aged 3 to 5 in publicly supported care. Our measure of childcare as the percentage of children enrolled is the standard measure used by researchers (Gornick & Meyers, 2003; Gornick et al., 1997; Hook, 2006; Kamerman & Kahn, 1991; Lewis, 2009; Mandel & Semoyonov, 2005, 2006; Pettit & Hook, 2005, 2009) and shows substantial variation, particularly for children aged 0 to 2.[16] For leaves, our measures distinguish between well-paid maternity and paternity leaves and generally low-paid or unpaid job-protected parental care leaves that begin after maternity leave is exhausted. We include only statutory, job-protected leave provisions that can be taken full time. We see variation in all of our measures, though less so in paternity leave, which may serve more as a signal of cultural valuation regarding the importance of father-care and gender equitable care sharing. Of course, these policy measures capture only access to federal leaves; for example, some American professional workers have access to leaves through their workplaces. Yet, our focus is on federal policy, not workplace policies; in addition, differential access matters less in other contexts, where all workers are covered by federal policy.

Our last policy indicator is a measure of tax disincentives to (married) women's employment participation: This measure represents the proportion of the second earners' income that pays for the increased income taxes in a dual-earner household where the first earner's wages equals 100% of average production workers' wages, and the second earner would go from earning no wages to earning 100% of average production workers' wages as well (Jaumotte, 2003b). While we do not have data on this measure for Israel, Russia, or the Slovak Republic, we observe considerable variation among the 19 remaining countries. Finally, we use a set of country-level control variables to conduct a robustness analysis of our policy models, which are also included in Table A2 of the Appendix. These include maternal employment rates, size of the public sector, and level of overall income inequality as measured by the Gini coefficient. Public sector employment is from the International Labour Organization (ILO, 2012).[17] All other measures were calculated based on LIS data.

Methodological Approach

Differences in the motherhood penalty in earnings across countries could be due to differential selection of mothers into employment across countries. To control for this differential selection, we use a two-stage Heckman selection correction approach to estimate the inverse Mills ratio (IMR) for inclusion as a selection criterion in our models (Heckman, 1979). We estimate the IMR prior to estimating our multilevel models: We first estimate separate probit regressions in each country that predict employment using nonfamily status-based transfer income,[18] other household labor market income (household earnings from employment minus respondent's earnings), and presence of a preschooler as selection criteria. Because Heckman corrections can inflate standard errors due to collinearity between the correction term and the included regressors (Moffitt, 1999; Stolzenberg & Relles, 1990), we include nonfamily transfer income as our instrumental variable that meets the exclusion restriction (it is not included in the equation estimating earnings). We argue that the extent to which an individual can rely on government transfers, as opposed to earnings, for income should affect her propensity for employment but is unlikely to affect her earnings if employed. Because we include benefit levels for family-related transfers in some models, we exclude them from the measurement of this instrumental variable. From the results of these models, we derive the IMR, which we then include as an individual-level predictor variable in all multilevel models, as the second step of Heckman's correction.

Multilevel modeling enables direct tests of the relationships between societal-level factors and individual-level effects while simultaneously modeling individual and contextual controls, as well as correctly estimating standard errors with our data where individuals are nested within countries (DiPrete & Forristal, 1994; Raudenbush & Bryk, 2002).[19] Multilevel models estimate the impact of country-level and individual-level factors simultaneously and can be written as follows:

$$\begin{aligned}\mathrm{Earnings}_{ij} = \gamma_{00} + \gamma_{10} \times \mathrm{NUMKID} + \gamma_{11} Z_j \\ \times \mathrm{NUMKID} + \gamma_{01} Z_j + \gamma_{20} X_{ij} + u_{0j} + r_{ij}\end{aligned}$$

where i indices individual women and j indices country. $\mathrm{Earnings}_{ij}$ represent log of individual i's annual earnings in country j. β_{0j} is the intercept, denoting mean earnings. Number of children, and its coefficient $\beta_{1j,}$ estimates the average per-child motherhood penalty across all countries. X_{ij} is the vector of other individual measures (marital status, human capital, job characteristics, etc.), and β_{2j} is the vector of their coefficients. r_{ij} is the individual-level error term. The γ coefficients represent country-level coefficients, Z_j the vector of country-level measures (policy and cultural), and u_j the country-level residuals. Note that only the equation for the intercept β_{0j} has an error term, that is, we use random-intercept models. The coefficient of interest is the cross-level interaction $\gamma_{11}Z_j \times \mathrm{NUMKID}$, which estimates the relationship between social policies and the number-of-children slope, that is, the average per-child motherhood penalty across all individuals in all countries. We chose random-intercept models as opposed to random-slopes models due to our limited degrees of freedom at the country level and because the per-child motherhood penalties are all in the same direction (the effect in Israel, albeit positive, is close to zero and nonsignificant as illustrated in Figure 2).[20] All Level-1 covariates are modeled as fixed effects, assuming that the direction of their effect is the same across all countries.

Modeling Strategy

To estimate the relationships displayed in Figure 1 and to test our hypotheses, we use a nested modeling approach. We begin in Model 1 with two variables: the natural log of annual earnings and number of children. This model shows the total, or gross, effect of children on earnings and serves as our baseline model. Models 2 and 3 add individual-level covariates. Model 2 (and all subsequent models) includes marital status, age, work hours, education, and the selection control (IMR). Model 3 adds job characteristics argued to serve as compensating differentials for the motherhood penalty; however, we consider these characteristics to be endogenous to motherhood and earnings and do not include them in the multilevel models.

Beginning with Model 4, we include country-level measures and their interactions to examine whether policy indicators shape the motherhood penalty cross-nationally. Models 4 and 5 test our first hypothesis that childcare should be inversely related to the motherhood wage penalty. Model 4 adds to Model 2 the measure of 0 to 3 year olds in publicly funded childcare as a main effect and as an interaction with number of children, while Model 5 adds the measure for 3 to 6 year olds. Significant and positive interactions of these childcare measures with number of children would support our first hypothesis, as the effect of children is a negative effect.

Models 6 to 9 are designed to test our second set of hypotheses regarding leave and the motherhood penalty. Models 6 and 7 include the number of weeks of paid maternity and paid paternity leave, respectively. We predict that these short-term paid leaves will be inversely related with the motherhood penalty in Hypothesis 2a. Model 8 tests parental leave generosity, which multiplies level of pay reimbursement by the length of leave. Model 9 is designed to test our Hypothesis 2b that predicted that extended parental care leave (including unpaid leave) would have a curvilinear relationship with the motherhood penalty. Here, we include number of weeks of extended leave and its squared term and interact both with the motherhood penalty.

Model 10 tests our third hypothesis that predicts higher tax penalties for second earners will correlate with greater motherhood penalties. Here, we include our measure of the proportion of the second earner's wage required to pay additional taxes as a main effect and interact this with number of children. Finally, Models 11 and 12 include two policy indicators in the same model to test whether findings are robust when tax policy and childcare (Model 11), or tax policy and parental leave (Model 12) are considered together.

Robustness Analysis

In our final analysis, we reestimate all 12 models described earlier and include other country-level factors that may account for the observed policy relationships with the motherhood penalty. We first include maternal employment rates as a measure of country-specific differences in employment opportunities for mothers. Women's labor force participation is the lowest in Italy and Spain. If, due to positive selection into the labor force, the mothers more likely to earn less are not in the labor markets in these countries, we might find lower motherhood penalties. Second, we include a measure of the proportion of workers in a country, who are located in the public sector. Generally, the public sector is more likely to enforce work–family policies that could reduce the motherhood penalty (Nielsen, Simonsen, & Verner, 2004). Third, we include the Gini coefficient as a measure of income inequality, drawn from the LIS key figures. It may be that countries with larger motherhood penalties simply have greater overall income disparities, similar to the impact of income inequality on gender gaps in earnings (Blau & Kahn, 1992, 1996, 2003; Mandel & Semyonov, 2005). And finally, in the robustness analysis, we control for gross domestic product (GDP) per capita to account for the persistent differences in overall wealth especially between the Eastern and Western countries included in our sample.

Potential Limitations

Our models address endogeneity that may occur, for example, if women who are more likely to have low earnings are more likely to have children, therein reversing the causal order of the logic of the motherhood penalty from (a) children causing reduced earnings to (b) low earnings leading to motherhood. While establishing causal order is difficult with cross-sectional data, we include all available measures of human capital (education and age, as potential experience), labor supply, and family composition (including marriage) in our models.

Despite the individual-level control variables included in our models, unobserved heterogeneity among women within and between countries may constrain our ability to fully explain variation in the motherhood penalty and the full effects of policies on this penalty (but see Waldfogel, 1998b, showing that controlling for unobserved heterogeneity does not lower the motherhood penalty in a cross-national study). For example, differences in women's preferences regarding employment and motherhood are unobserved in our data. Cross-sectional data prevent us from controlling for stable unmeasured heterogeneity through statistical models, and this is a limitation of our data. However, it is reasonable to think that family policies, in addition to directly impacting the motherhood penalty, may also alter the sociopolitical norms regarding employment among mothers, which, in turn, may change women's own preferences and thereby affect the motherhood penalty. Hook (2006) makes a similar argument about the impact of social policies influencing normative gendered behaviors. Similarly, policy contexts may impact employers' preferences for hiring and evaluating the work performance of mothers. To the extent policies change preferences, this kind of unobserved heterogeneity would be difficult to capture even with longitudinal data in the absence of measures of preferences. Despite these limitations, our study advances the state of knowledge and leads us closer to designing future studies to address causality.

A third limitation of our study design is the restricted Level-2 sample size or that our Level-2 *N* is limited to 22 countries. Research demonstrates that multilevel models produce stable coefficients with fewer than 15 macrocases (Quillian, 1995; Raudenbush & Liu, 2000). Indeed, multilevel models have been used with the LIS data to examine the effects of welfare policies on the gender gap in earnings for 14 to 20 countries (Mandel & Semyonov, 2005) and the effects of work–family policies on women's employment for 19 countries (Pettit & Hook, 2005). Yet, the small *N* increases the chances of a type-2 error (failing to find significance for a nonrandom relationship). This implies that we can have greater confidence in any significant relationships we do find; we are not at risk of misidentifying as significant relationships that are not significant, but instead, our risk is of not finding relationships where they exist. Due to the small number of Level-2 cases, we do have limited power to estimate random-slopes models (letting the effect of the number of children vary across countries); we therefore estimate random-intercept models, where only average earnings (intercept) is allowed to vary across countries. Finally, a small N reduces the number of Level-2 control variables that can be modeled simultaneously. Consequently, we do a robustness analysis controlling for country-level factors by entering them separately. Despite these limitations, multilevel models have been used with the LIS data to examine the effects of welfare policies on the gender gap in earnings for 14 to 20 countries (Mandel & Semyonov, 2005) and the effects of work–family policies on women's employment for 19 countries (Pettit & Hook, 2005).

Findings

The Earnings Penalty for Motherhood

We begin our series of nested multilevel models with a model that estimates the total effect of the number of children in the household on earnings. Model 1 of Table 1 shows that the unadjusted average child effect across countries is statistically significant ($p < .000$) and that women lose about 15% in annual earnings per child, $(e^{0.16} - 1) \times 100$.[21] Model 2 adds marital status, human capital characteristics, and accounts for selection into employment by including the IMR, which reduce the average per-child penalty by 57%, from 15% to 8% $(e^{.088} - 1)$, but the penalty remains significant. The standardized coefficients, presented in the Beta column, show that being a part-time worker has the strongest (negative) association with earnings.[22] Because mothers are far more likely to work part time compared with childless women, controlling for this variable accounts for a significant proportion of the motherhood earnings penalty. Our third model adds job characteristics, and, consistent with past findings (Budig & England, 2001), we observe that adding these characteristics does not explain the child penalty. Because we believe job characteristics are endogenous to the earnings equation, our next series of models that estimate policy effects use only human capital and family structure controls.

Table 1. Effects of Individual-Level Covariates and Country-Level Childcare Measures on Women's Ln Annual Earnings, Unstandardized (B) and Standardized Coefficients (Beta) From Multilevel Models.

	1		2		3		4	5
	Gross penalty		Human capital		Job characteristics		Human capital + enrollment of 0 to 3 year olds	Human capital + enrollment of 3 to 6 year olds
	B	Beta	*B*	Beta	*B*	Beta	*B*	*B*
Number of children	**−0.160**	**−.145**	**−0.088**	**−.080**	**−0.084**	**−.077**	**−0.101**	**−0.119**
Married/Cohabiting			0.014	.005	0.010	.003	0.013	0.014
Age			**0.017**	**.081**	**0.016**	**.077**	**0.017**	**0.017**
Part-time worker			**−0.743**	**−.230**	**−0.730**	**−.226**	**−0.741**	**−0.743**
Higher education			**0.508**	**.174**	**0.300**	**.103**	**0.507**	**0.508**
Inverse Mills ratio			**−1.923**	**−.109**	**−1.892**	**−.107**	**−1.919**	**−1.927**
Professional-managerial worker					**0.421**	**.140**		
% Women in occupation					0.015	.002		
% 0 to 3 year olds in childcare							−0.003	

(continued)

Table 1. (continued)

	1		2		3		4	5
	Gross penalty		Human capital		Job characteristics		Human capital + enrollment of 0 to 3 year olds	Human capital + enrollment of 3 to 6 year olds
	B	Beta	*B*	Beta	*B*	Beta	*B*	*B*
% of 0 to 3 year olds in childcare × number of children							**0.001**	
% 3 to 6 year olds in childcare								−**0.**002
% of 3 to 6 year olds in childcare × number of children								**4.93E-04**
Intercept	**9.154**		**9.228**		**9.208**		**9.256**	**9.340**
BIC	173,632		157,833		155,424		157,847	157,869
AIC	173,595		157,741		155,315		157,737	157,759

Note. Bolded coefficients are significant at the .05 significance level (two-tailed test). BIC = Bayesian information criterion; AIC = Akaike information criterion.

The fourth model in Table 1 presents the cross-level interactive effect between the percentage of children aged 0 up to 3 years who are in publicly funded childcare slots and the per-child penalty. Our first hypothesis posited that the availability of state-provided childcare should reduce the negative effect of children on women's earnings by mitigating lost job experience due to childrearing. The findings support our hypotheses. In this model, the main effect for number of children is $-.101$, indicating that, in a country with no children aged $0 \leq 3$ years enrolled in publicly funded childcare, the per-child penalty is 9.6%. The significant interaction between infant childcare and number of children is positive and equals .001. This indicates that each additional percentage of infants in publicly funded care is associated with a .001 decline in log points for the motherhood penalty. We see in the fifth model, turning to care for older preschoolers, findings are similar as for infant care, though weaker in size.

To show the impact of these interacted effects more clearly, Figure 3 presents the per-child association with earnings across the observed cross-national distribution of the percentage of infants (solid line) and older preschoolers (dashed line) in publicly funded care. We see that higher enrollments of 0 to 3 year olds in public childcare are associated with smaller penalties, reducing the per-child penalty from 9.5% in countries with only 1% of children in such care to 4.3% in countries with 41% of infants in publicly subsidized care. We do not extrapolate outside of our observed values: Sweden, the country with the highest percentage of infants in publicly funded care, has 41% of infants in public care.[23] Similarly, we see that in countries with the lowest observed percentage of children aged 3 up to 6 in public care (39%), the associated wage penalty is 9.5% per child. At the highest levels of enrollment for this age-group, 99%, the per-child penalty is reduced to 6.8%. That the strength of the associations of older preschooler care with the motherhood penalty is weaker than that of infant care is not surprising. In many countries, childcare for this older age-group is part of the early education system, is more focused on its educational aspects than its efforts to help families balance work and family demands, and does not correspond with normal working hours. In summary, our first hypothesis is firmly supported: Greater levels of childcare for infants and preschool children are linked to smaller motherhood penalties.

Turning to the impact of family leave, we first consider paid leaves: paid maternity leave, paid paternity leave, and our calculated measure of weeks of fully funded job-protected parental care leaves. In Model 6 in Table 2, we find a significant and positive interaction between number of children and weeks of fully paid maternity leave. Figure 4 shows how

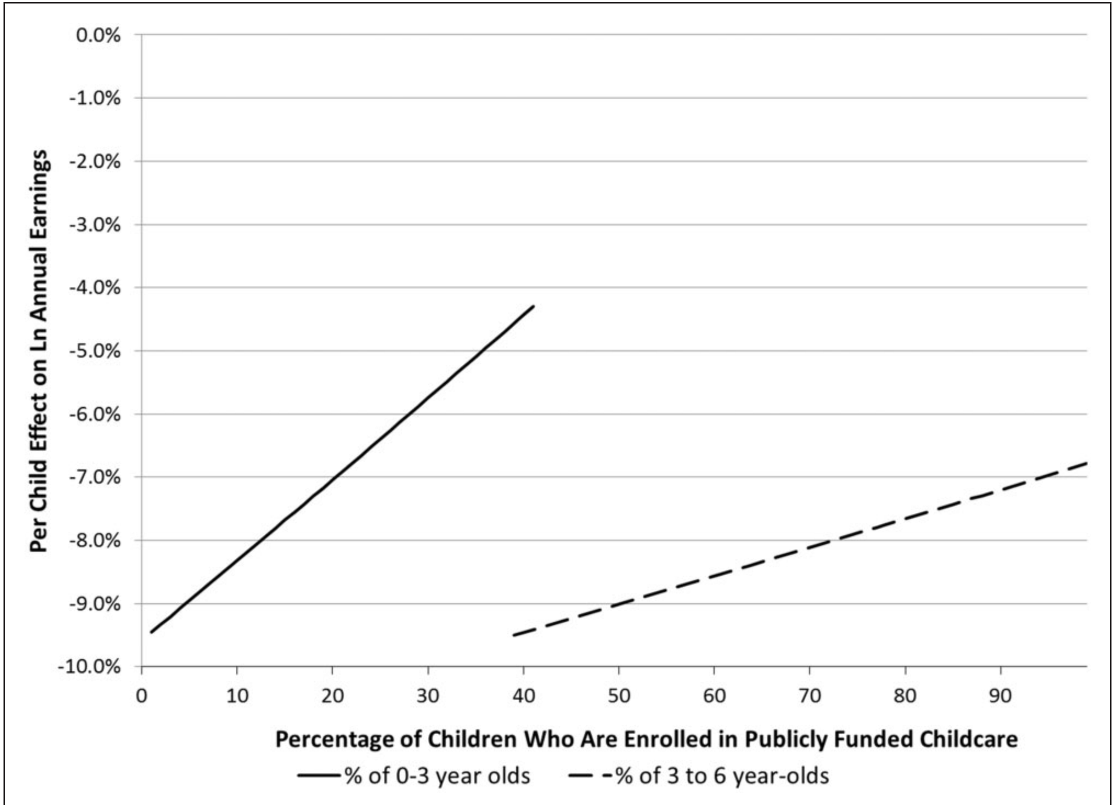

Figure 3. Net per-child effect on logged annual earnings, by percentage of children aged 0 to <3, and for 3 to 6 enrolled in publicly funded childcare.

Table 2. Effects of Maternity, Paternity, and Parental Leave on Women's Ln Earnings, Unstandardized Coefficients From Multilevel Models.

	6 Maternity leave generosity	7 Weeks of paid paternity leave	8 Parental leave generosity	9 Maximum weeks of leave
Number of children	**−0.111**	**−0.096**	**−0.098**	**−0.141**
Married/Cohabiting	0.013	0.014	0.013	0.012
Age	**0.017**	**0.017**	**0.017**	**0.017**
Part-time worker	**−0.742**	**−0.741**	**−0.741**	**−0.741**
Higher education	**0.508**	**0.507**	**0.507**	**0.506**
Inverse Mills ratio	**−1.926**	**−1.920**	**−1.927**	**−1.922**
Weeks of fully paid maternity leave	**−0.115**			
Weeks of fully paid maternity leave × number of children	**0.002**			
Weeks of paid paternity leave		0.122		
Weeks of paid paternity leave × number of children		**0.017**		
Weeks of fully paid parental leave			−0.018	

(continued)

Table 2. (continued)

	6 Maternity leave generosity	7 Weeks of paid paternity leave	8 Parental leave generosity	9 Maximum weeks of leave
Weeks of fully paid parental leave × number of children			**0.001**	
Maximum length of leave for women				0.001
Maximum length of leave for women × number of children				**0.002**
Maximum length of leave for women squared				−5.93E-05
Maximum length of leave for women squared × number of children				**−1.03E-05**
Intercept	**10.763**	**9.152**	**9.540**	**9.910**
BIC	157,814	157,800	157,848	157,854
AIC	157,705	157,690	157,738	157,726

Note. Bolded coefficients are significant at the .05 significance level (two-tailed test). BIC = Bayesian information criterion; AIC = Akaike information criterion.

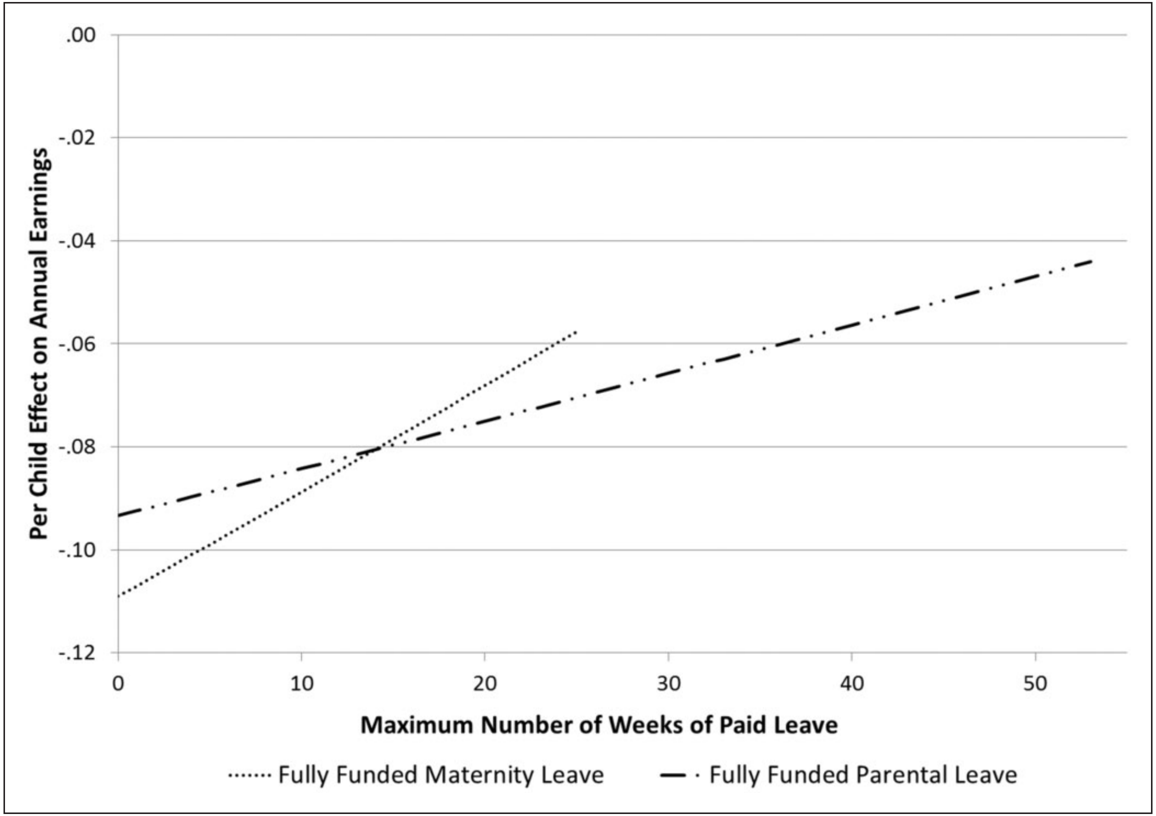

Figure 4. Net per-child effect on logged annual earnings by number of weeks of fully paid maternity leave and fully funded parental leave.

the per-child effect changes by length of paid maternity leave such that, in countries with 0 weeks of paid maternity leave, the predicted per-child penalty is 11% and shrinks to 6% per child as paid maternity leave length increases to 25 weeks. This finding supports Hypothesis 2a in stating that paid maternity leaves should be negatively associated with the size of the motherhood penalty.

Paid paternity leave also shows a significant and positive interaction with number of children, indicating that where paid paternity leave lengths are greater, the motherhood penalty is smaller. Model 7 of Table 2 shows the cross-level interactive effect between number of children and length of paternity leave. Here, we find that the average per-child effect in countries offering no paid leave to fathers is about 9.2% per child. The significant interaction is positive, however, and shows that for each additional week of paternity leave, the per-child penalty declines by about 1.7% points. While this implies that 6 weeks of paid paternity leave might eradicate motherhood penalties, we urge caution against interpreting this effect in such a manner. The vast majority of countries offer no paid paternity leave to men (see Table A2 of Appendix), and several of those that do offer only a few days. In summary, we find some evidence to support our Hypothesis 2a in regard to paternity leave, though we caution against overinterpretation of this evidence.

Next, we consider the effect of our calculated measure for number of fully funded weeks of leave (weeks of job-protected parental leave times benefit level). As the cross-level interaction between fully funded leave length and number of children in Model 8 in Table 2 and Figure 4 shows, weeks of fully funded parental leave also significantly impact the size of the motherhood wage penalty. The per-child penalty in countries with 0 weeks of funded leave is 9.3%, and this declines to a minimum of 4.4% in countries with 53 fully funded weeks of parental care leave.[24] Overall, Hypothesis 2a is strongly supported by our findings for paid maternity and paid parental care leaves, with some support for paid paternity leaves as well.

In addition to paid leaves, we also examined the cross-level effects of the combined duration of job-protected maternity parental care leave for women with the motherhood penalty. The ninth model in Table 2 presents the impact of the maximum number of weeks of women's job-protected parental leave (and its squared term) on the per-child penalty for motherhood.[25] Here, we test Hypothesis 2b, which stated that the duration of care leave, regardless of benefit level, should have curvilinear associations with the motherhood penalty. The main effect of number of children is significant and indicates that in countries with 0 weeks of job-protected leave (though none exist), the per-child penalty would be roughly 13.2%. The cross-level statistical interaction between the child penalty and weeks of leave is significant ($p \leq .001$, two-tailed test) and positive, while the cross-level interaction between the child penalty and the squared leave term is significant and negative. This indicates a curvilinear relationship, which is best viewed graphically in Figure 5. This figure shows how the effect of children on earnings varies by the number of weeks of leave offered to women as a solid line. The curvilinear pattern is dramatic and shows that countries with 0 to 49 weeks of job-protected care leave have very large motherhood penalties (exceeding 6% per child), as do countries with extremely long leaves, from 157 to 173 weeks (3 or more years). But even the longest leaves are associated with smaller penalties relative to no leave at all. Leave lengths between 50 and 156 weeks are predicted to be associated with the smaller motherhood penalties.

In summary, for women, we find our Hypothesis 2b supported for extended care leaves. Countries that allow for very long leaves of absence (3 or more years) are also associated with high motherhood penalties, perhaps due to lost human capital or employer discrimination against long-absent workers. Still, countries with very short leave provisions show the largest motherhood penalties. However, countries that allow for 2 years of job-protected leave are associated with the smallest per-child penalties—that are roughly 73% smaller relative to no leave, perhaps because this leave length strikes the best balance between mothers' desires to care for newborns and their desires to return to employment. While ideally, we would present childcare and leaves together in the same model, we are not able to do so due to multicollinearity.

We next consider whether motherhood penalties are larger when the marginal tax rate of the second earner's income is higher. Here, we test Hypothesis 3, which stated that taxation policies that penalize second earner's incomes in coupled households should be related to higher motherhood earnings penalties by encouraging interrupted attachment to the labor market and reduced experience due to childbearing. Model 10 of Table 3 and Figure 6 show evidence in support of our third hypothesis: The percentage of the second earner's income that is needed to pay the additional income tax generated by that income is significantly related to the size of the motherhood penalty. Countries with the lowest reported marginal tax rates,

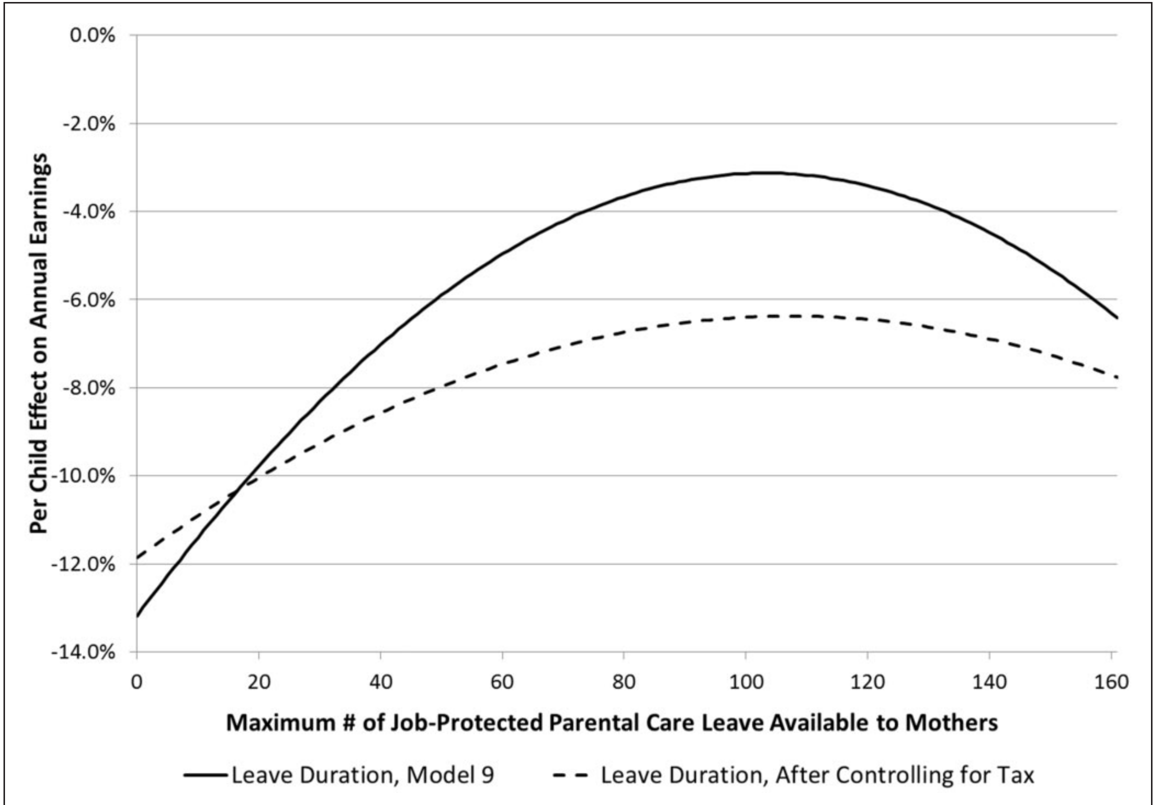

Figure 5. Net per-child effect on logged annual earnings by the maximum number of weeks of job-protected leave available to mothers.

Table 3. Effects of Tax Disincentives for Second Earners and Combinations of Policy Indicators on Ln Women's Earnings, Unstandardized Coefficients From Multilevel Models.

	10	12	13
	Taxation of second earner's wage (100% of APW)	Taxation and childcare	Taxation and leave
Number of children	−**0.047**	−**0.050**	−**0.055**
Married/Cohabiting	**0.018**	**0.017**	0.016
Age	**0.019**	**0.019**	**0.018**
Part-time worker	−**0.742**	−**0.740**	−**0.740**
Higher education	**0.514**	**0.513**	**0.513**
Inverse Mills ratio	−**2.023**	−**2.021**	−**2.026**
Taxation at 100% of APW	0.009	0.007	0.014
Taxation at 100% of APW × number of children	−**0.002**	−**0.002**	−**0.002**
% 0 to 3 year olds in childcare		0.006	
% of 0 to 3 year olds in childcare × number of children		**0.001**	
Maximum length of leave available to women			−0.002
Maximum length of leave available to women × number of children			**0.001**

(continued)

Table 3. (continued)

	10	12	13
	Taxation of second earner's wage (100% of APW)	Taxation and childcare	Taxation and leave
Maximum length of leave available to women squared			−2.69E-05
Maximum Length of Leave available to women squared × number of children			**−6.45E-06**
Intercept	**9.150**	**9.170**	**9.482**
BIC	143,852	143,878	143,916
AIC	143,744	143,751	143,772

Note. Bolded coefficients are significant at the .05 significance level (two-tailed test). APW = average production workers' wages; BIC = Bayesian information criterion; AIC = Akaike information criterion.

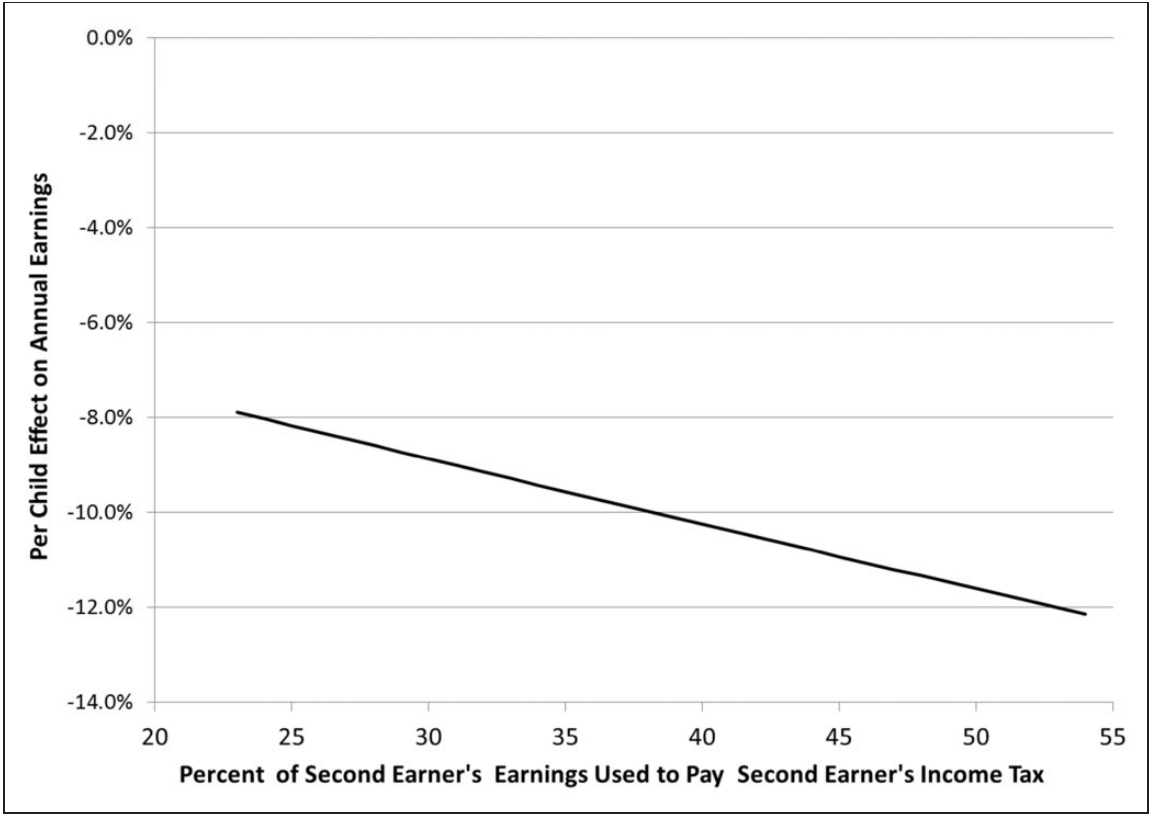

Figure 6. Net per-child effect on logged annual earnings by average percentage of second earner's earnings used to pay second earner's income taxes.

of 23%, show the smallest per-child penalties of 7.9%. Per-child penalties become larger as the marginal tax rate increases, such that the highest marginal tax rate of 53% is associated with a 12.2% child penalty.

The level of taxation of the second earner's income may have trade-offs with usage of work–family policies, particularly childcare for very young children and lowly benefitted or unpaid job-protected leave. To consider whether the relationship of childcare and leave policy indicators are contingent on second earner tax rates, in Models 12 and 13 of Table 3, we reestimate our earlier models for childcare of 0 to 3 year olds and for duration of parental leave by including taxation rates of second earners. In Model 12, the size and significance of the cross-level interactions between number of children with childcare for 0 to 3 year olds is unchanged with the inclusion of the taxation policy, as is the cross-level interaction of taxation with number of children unaffected by the inclusion of the childcare rates of 0 to 3 s. However, in Model 13, we find that the size and significance of the cross-level interaction between taxes and number of children becomes stronger, and the significance of leave duration and children becomes weaker, when we include both measures and their interactions in the model. This implies that the effects of leave duration may be partially accounted for by the degree of the tax penalty for second earners. While the two measures are uncorrelated in a bivariate analysis, they may be correlated after adjusting for the effects of other factors in the model and in terms of their joint prediction of the dependent variable (earnings).

To examine this, we reestimated the leave duration model including the taxation measure as an additive control. Results of this model are shown in Figure 5. Once we control for the tax penalty on the second earner's income, that leave duration still shows a curvilinear relationship where the absence of leave and very long leaves are associated with larger motherhood penalties, and the penalties are generally larger and less curvilinear, with the exception of leave durations of 14 weeks or less being associated with smaller motherhood penalties when the tax penalty is controlled. Thus, while the leave duration is less effective at moderating the impact of children on wages when the tax wedge is included, motherhood penalties are greater at almost every level of leave in this model.

Robustness of Cross-Level Interactions

We examine whether other salient country-level characteristics might explain the significant policy effects on the motherhood penalty established in our multilevel models. We thus conduct a robustness analysis of our significant interactions to examine whether the associations between policies and the child penalties might be due to country-level differences in women's labor force participation,[26] the size of the public sector, the level of within-country income inequality, and the size of the country's GDP. Table 4 presents the results from this series of analyses.

The four policy measures are presented in separate panels. The first column of Table 4 replicates results from Tables 1, 2, and 3. In results columns 2 through 5, we include each of the country-level control variables successively, in addition to human capital individual-level controls, the country-level policy measure, and its interaction with number of children. Column 2 includes a measure of women's labor force participation for each country. The third column controls for the size of the public sector in each country, associated both with maternal employment and the likelihood of enforcement of family policies. The fourth column controls for the Gini coefficient to examine whether country-level income inequality explains variation in the motherhood penalty and the effects of policies on this penalty. Finally, the fifth column controls for each country's GDP to examine whether the size of the country's economic growth is related to the size of the motherhood penalty and the effect of work–family policies on this penalty. In each model, the country-level control is included as a main effect, and we tested for statistical interactions with number of children. None of the country-level control variables significantly interacted with number of children, and nonsignificant interactions were excluded from the models.

This analysis reveals that our results are robust: In every model, results for the motherhood penalty and for the cross-level interaction between the policy indicator and the motherhood penalty are impervious to the inclusion of these country-level controls. Looking first at the effect of childcare for 0 to 3 year olds on the motherhood penalty, we find that none of the Level-2 control variables altered (a) the effect of children on earnings or (b) the interactive effect between childcare and number of children. While the main effects of the policy indicator

Table 4. Robustness Analysis: Multilevel Estimates of Policy Indicators' Effects on the Earnings Penalty for Motherhood Among Women Aged 25 to 45, Net of Individual-Level Controls and Country-Level Controls.

	Human capital controls only	+ Women's employment rates	+ % of Workers in public sector	+ Gini coefficient	+ GDP per capita
Early childhood education and care					
Effect of 0 to 3 childcare					
Number of children	**−.101**	**−.101**	**−.101**	**−.101**	**−.101**
% 0 to 3 in public care	−.003	.013	.025	−.009	−.006
% 0 to 3 in care × number of children	**.001**	**.001**	**.001**	**.001**	**.001**
Effect of 3 to 6 childcare					
Number of children	**−.119**	**−.119**	**−.119**	**−.119**	**−.119**
% 3 to 6 in public care	−.002	.001	.016	−.013	−1.17E−04
% 3 to 6 in care × number of children	**4.93E−04**	**4.93E−04**	**4.93E−04**	**4.93E−04**	**4.93E−04**
Leave policies					
Number of weeks of fully paid maternity leave					
Number of children	**−.111**	**−.111**	**−.111**	**−.111**	**−.111**
Weeks of fully paid maternity leave	**−.115**	**−.115**	**−.072**	**−.122**	**−.057**
Weeks of maternity leave × number of children	**.002**	**.002**	**.002**	**.002**	**.002**
Number of weeks of fully paid parental leave					
Number of children	**−.098**	**−.098**	**−.098**	**−.098**	**−.098**
Weeks of parental leave	−.018	−.015	.002	**−.029**	−.010
Weeks of parental leave × number of children	**.001**	**.001**	**.001**	**.001**	**.001**

(continued)

Table 4. (continued)

	Human capital controls only	+ Women's employment rates	+ % of Workers in public sector	+ Gini coefficient	+ GDP per capita
Maximum number of weeks of leave available to women					
Number of children	**−.141**	**−.141**	**−.141**	**−.141**	**−.141**
Maximum length of leave	.001	.000	.004	−.005	.002
Maximum length of leave × number of children	**.002**	**.002**	**.002**	**.002**	**.002**
Maximum length of leave squared	−5.93E−05	−5.42E−05	−5.02E−05	−2.79E−05	−2.68E−05
Maximum length of leave squared × number of children	**−1.03E−05**	**−1.03E−05**	**−1.03E−05**	**−1.03E−05**	**−1.04E−05**
Number of weeks of paternity leave					
Number of children	**−.096**	**−.096**	**−.096**	**−.096**	**−.096**
Weeks of paternity leave	.122	.104	.080	.118	.072
Weeks of paternity leave × number of children	**.017**	**.017**	**.017**	**.017**	**.017**
Taxation					
Increase in taxes paid by household where first earner earns 100% of APW and second earner, starts to earn 100% of APW in % of second earner's wages					
Number of children	**−.047**	**−.047**	**−.047**	**−.047**	**−.047**
Taxation of second earner's wage	.009	.010	.014	.006	.009
Taxation of second earner's wage × number of children	**−.002**	**−.002**	**−.002**	**−.002**	**−.002**

(continued)

Table 4. (continued)

	Human capital controls only	+ Women's employment rates	+ % of Workers in public sector	+ Gini coefficient	+ GDP per capita
Combinations					
Childcare and taxation					
Number of children	**−.050**	**−.050**	**−.050**	**−.050**	**−.050**
% 0 to 3 year olds in childcare	.006	.010	**.028**	1.54E−05	−.001
% of 0 to 3 year olds in childcare × number of children	**.001**	**.001**	**.001**	**.001**	**.001**
Taxation of second earner at 100% of APW	.007	.008	.005	.006	.009
Taxation × number of children	**−.002**	**−.002**	**−.002**	**−.002**	**−.002**
Leave and taxation					
Number of children	**−.055**	**−.055**	**−.055**	**−.055**	**−.055**
Maximum length of leave available to women	−.002	−.001	.001	−.007	−.003
Maximum length of leave × number of children	**.001**	**.001**	**.001**	**.001**	**.001**
Maximum length of leave available to women squared	−2.69E−05	−3.16E−05	−3.75E−05	−4.09E−08	−1.14E−07
Maximum length of leave squared × number of children	−6.45E−06	−6.45E−06	−6.46E−06	−6.45E−06	−6.46E−06
Taxation at 100% of APW	.014	.017	.016	.011	.012
Taxation × number of children	**−.002**	**−.002**	**−.002**	**−.002**	**−.002**

Note. Bolded coefficients are significant at the .05 significance level (two-tailed test). APW = average production workers' wages; GDP = gross domestic product.

(percentage of 0 to 3 year olds in public care) do change across models, the main effect for the motherhood penalty and its cross-level interaction with percentage of children in public care is unchanged.

The robustness checks for the other policy measures (paid leaves, parental care leave duration, tax policies, and combinations of indicator models) show equally resilient results. None of the control variables (employment probabilities, public sector, and the Gini coefficient) significantly interacted with number of children. Moreover, the impact of number of children on earnings, and the interactions between number of children and the policy measures, were unaffected by the inclusion of the country-level control variables. We thus conclude that our policy findings are robust to the inclusion of these country-level controls.

Discussion

Consistent with our first hypothesis, the increased prevalence of publicly funded childcare for children under the age of 3 and children aged 0 to 3 is significantly associated with smaller per-child penalties, despite the varying policy contexts of the 22 nations in our analysis. Programs for children under 3 have been explicitly designed to help parents maintain employment, while programs for children aged 3 to 6 are more often designed as educational programming in addition to supporting working parents (Gornick & Meyers, 2003; Kamerman & Kahn, 1991; Morgan, 2005).[27] Similarly, our Hypothesis 2a is confirmed in that the effects of leave provide evidence in support of our hypotheses that paid leaves (maternity, paternity, and equivalent fully paid weeks of parental care) all are inversely associated with the motherhood penalty: Where these paid leaves are longer, motherhood penalties are smaller. However, we urge caution in interpreting the effects of paternity leaves, which are very short. With such little time offered to fathers, we think it more likely that the presence of paid paternity leave may signal cultural differences in the valuation of father involvement with children and an emphasis on more gender equitable sharing of care. Indeed, Sweden and Finland are known for their multiple policies aimed at gender egalitarianism, and paternity leave may be a signal of a broader regime of equalizing the sexes. In regard to the duration of extended parental leaves, we find a curvilinear relationship as predicted by Hypothesis 2b: Both the absence of care leaves and very long leaves for women serve to increase the negative effects of motherhood on earnings, while moderate job-protected leaves are associated with smaller motherhood penalties. Very long extended leaves may indeed (like short leaves) create trade-offs for women—but this is not a paradox; such leaves reflect gendered cultural ideas regarding maternal caregiving (Kremer, 2007) that sensibly may be associated with lower maternal wages.

Consistent with our Hypothesis 3, tax policies also matter. Where the marginal income tax rate for the second earner captures a greater share of her earnings, motherhood penalties are larger. Moreover, marginal second earner tax rates influence the relationship between leave duration and the motherhood penalty, such that leave has weaker relationship with the motherhood penalty in models where we adjust for the second earner tax rate. Given that this measure is a conservative one—as these tax rates may influence employment decisions for childless women as well as mothers—we suspect that recognizing the influence of tax policies may be even more important for understanding gender gaps in wages.

Importantly, all of our findings were robust to the inclusion of other country-level factors. Notably, the Gini coefficient, while having a negative and significant impact as a main effect, failed to alter the relationship between children and earnings. This is very interesting, particularly given the important impact of income inequality on gender gaps in earnings (Blau & Kahn, 1992, 1996, 2003), and suggests that motherhood penalties (as opposed to gender gaps) cannot be explained or easily attributed to larger economic pressures leading to earnings inequalities. Our findings were also robust to alternate specifications of motherhood (using one, two, and three or more child dummy variables) and to combinations of policies (i.e., including taxation policies together in models with childcare and leave policies).

Our findings help explain the tremendous variation in the motherhood penalty, as shown in the enormous motherhood penalties in West Germany (see Figure 1), as opposed to the much smaller effect in Sweden, and the insignificant effect in Israel, controlling for human capital, labor supply, family structure, and selection into employment. As shown in Table A2, in West Germany during this period, only 5% of children aged 0 to 3 were in publicly subsidized care, as compared with 19% in Israel, or an even larger 41% in Sweden. Job-protected parental leave length was 64 weeks in Israel and Sweden, yet 161 weeks in West Germany. Taxation of the second earner's income was also substantially larger in West Germany than in Sweden. While West German policies have since changed, shifting to a leave scheme modeled on the Swedish policies, our results make clear that its previous policies helped exacerbate the motherhood wage penalty.

On the other hand, it may seem that the United States should have a larger penalty, given its very low levels of public childcare provisioning and its very short (12 weeks) job-protected parental leave, which is not even available to all workers. Yet, U.S. penalties also reflect relatively low taxations rates on the second earner's income, high rates of women's employment, and a low-skill, low-wage market childcare system that replaces public childcare, though in ways that sacrifice quality of programming for children and equality among families (Morgan, 2005).

Conclusions

Our analysis endeavored to accomplish several goals. We aimed to adjudicate debates within the literature regarding whether welfare state interventions create trade-offs. We note that much of this literature explores differences between men and women, even though many of the welfare state interventions are aimed at mothers, rather than all women. We then focused on explaining differences among women, based on how many children they have, to unpack the effects of welfare state interventions on these differences—or the motherhood penalty on wages.

Much previous research on the effects of welfare state interventions has either taken a broad comparative approach—comparing different countries and outcomes (Esping-Andersen, 1999; Gornick & Meyers, 2003; Orloff, 2002), or created work–family policy indices to capture overall state interventions (Mandel & Semyonov, 2005, 2006). We examine the relationship between particular policies and the motherhood penalty cross-nationally, arguing that it is important to measure policies separately and in ways that best capture their potential positive or negative effects. This allows us to argue that it is not that generous welfare states create trade-offs regarding gender equality but particular policies—such as extended parental leaves—that do so.

Our research breaks with the tradition of associating ideal welfare state types with women's economic outcomes. While this approach has advanced understandings of welfare states and gender inequities significantly, it cannot disentangle contexts particular to specific countries from their policy effects. Our approach reveals that, controlling for individual level differences, and despite significant differences in socioeconomic and political settings, some policy effects are quite robust. For policy makers contemplating which policies might be most effective at reducing pay inequities, the answer is clear: Policies that serve to keep women attached to the labor market, through moderate-length leaves, publicly funded childcare, lower marginal tax rates on second earner income, as well as support for father involvement after a birth, appear most effective at reducing the motherhood penalty.

Overall, we have integrated important insights made by scholars regarding how welfare state interventions via work–family policies and affect women's economic outcomes, as well as the factors that shape earnings penalties to mothers. By integrating these different literatures, we have been able to explain previously conflicting findings while identifying the ways in which policies should be constructed to promote the best outcomes for mothers' earnings. At their core, we believe that work–family policies are neither good nor bad—but have complex effects that relate to the gendered assumptions underlying the policies (Kremer, 2007). High-quality childcare and moderate paid leaves support mothers' employment; long-term care leaves, on the other hand, help weaken women's labor force attachment and may indeed increase employers' reluctance to hire mothers (Correll et al., 2007).

In addition, we believe that our findings have important implications for understanding gender inequality as well as motherhood penalties. Over time, earnings for childless men and women have been converging; yet, earnings for mothers and fathers remain significantly different in many countries. This means that unpacking the sources of inequality between mothers and childless women (as well as between fathers and childless men as in Glauber, 2007; Hodges & Budig, 2010) is an important step toward fully unpacking the sources of gender inequality. Future research should explicitly consider the degree to which parenthood generates observed gender earnings gaps.

Appendix

Table A1. Weighted Means and Standard Deviations (in Parentheses) for Individual-Level Variables.

		No. of children		Natural log of annual earnings		Married/ Cohabiting		Age		Part-time status		Higher educational attainment		Professional/ Managerial worker		Proportion of women in occupation	
	N	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless
Australia	1,441	1.974	NA	9.387	9.816	0.840	0.666	36.635	32.282	0.484	0.120	0.194	0.331	0.229	0.362	0.581	0.570
		(0.803)		(0.722)	(0.532)	(0.367)	(0.472)	(5.132)	(5.447)	(0.500)	(0.325)	(0.395)	(0.471)	(0.421)	(0.481)	(0.190)	(0.187)
Austria	545	1.659	NA	8.938	9.532	0.854	0.559	36.301	33.270	0.414	0.080	0.121	0.303	0.063	0.187	0.578	0.557
		(0.689)		(0.740)	(0.572)	(0.353)	(0.498)	(5.656)	(6.169)	(0.493)	(0.272)	(0.327)	(0.461)	(0.244)	(0.391)	(0.160)	(0.149)
Belgium	812	1.939	NA	9.433	9.639	0.874	0.594	36.945	32.123	0.314	0.157	0.447	0.554	0.232	0.209	0.595	0.569
		(0.852)		(0.753)	(0.547)	(0.333)	(0.492)	(5.173)	(5.944)	(0.465)	(0.365)	(0.498)	(0.498)	(0.423)	(0.408)	(0.158)	(0.174)
Canada	8,141	1.921	NA	9.332	9.664	0.842	0.581	36.880	33.630	0.249	0.122	0.176	0.309	0.191	0.218	0.633	0.612
		(0.833)		(1.152)	(0.958)	(0.364)	(0.493)	(5.417)	(6.269)	(0.432)	(0.327)	(0.381)	(0.462)	(0.393)	(0.413)	(0.216)	(0.220)
Czech Republic	6,907	1.829	NA	8.088	8.271	0.873	0.621	37.046	34.389	0.044	0.040	0.093	0.186	0.079	0.146	0.625	0.607
		(0.678)		(0.583)	(0.524)	(0.333)	(0.485)	(5.457)	(7.074)	(0.204)	(0.195)	(0.291)	(0.389)	(0.270)	(0.353)	(0.193)	(0.208)
Finland	2,752	1.957	NA	9.347	9.526	0.855	0.625	37.041	32.697	0.088	0.081	0.199	0.261	0.217	0.270	0.678	0.668
		(0.888)		(1.049)	(0.910)	(0.352)	(0.484)	(5.280)	(6.284)	(0.283)	(0.273)	(0.400)	(0.440)	(0.412)	(0.444)	(0.199)	(0.211)
France	2,723	1.826	NA	9.086	9.315	0.856	0.547	36.518	32.316	0.250	0.130	0.161	0.354	0.196	0.280	0.691	0.643
		(0.802)		(0.869)	(0.753)	(0.351)	(0.498)	(5.456)	(6.257)	(0.433)	(0.336)	(0.367)	(0.478)	(0.397)	(0.449)	(0.222)	(0.228)
East Germany	762	1.571	NA	9.326	9.306	0.811	0.473	36.976	32.244	0.187	0.164	0.373	0.404	0.116	0.122	0.672	0.664
		(0.673)		(0.927)	(1.107)	(0.391)	(0.501)	(5.201)	(6.267)	(0.390)	(0.371)	(0.484)	(0.492)	(0.320)	(0.329)	(0.225)	(0.243)
West Germany	2,466	1.755	NA	8.955	9.825	0.832	0.588	37.271	33.771	0.523	0.137	0.208	0.305	0.064	0.195	0.674	0.635
		(0.806)		(1.102)	(0.825)	(0.374)	(0.492)	(5.124)	(5.944)	(0.500)	(0.344)	(0.406)	(0.461)	(0.244)	(0.396)	(0.219)	(0.228)

(continued)

Table A1. (continued)

		No. of children		Natural log of annual earnings		Married/ Cohabiting		Age		Part-time status		Higher educational attainment		Professional/ Managerial worker		Proportion of women in occupation	
	N	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless
Hungary	406	1.808	NA	7.295	7.604	0.903	0.591	36.774	33.677	0.084	0.056	0.202	0.288	0.192	0.235	0.786	0.709
		(0.742)		(0.966)	(0.741)	(0.296)	(0.496)	(5.388)	(6.591)	(0.277)	(0.232)	(0.402)	(0.457)	(0.395)	(0.428)	(0.217)	(0.238)
Ireland	640	2.214	NA	9.223	9.810	0.868	0.625	36.522	30.957	0.378	0.184	0.217	0.506	0.153	0.332	0.626	0.590
		(1.024)		(0.873)	(0.757)	(0.338)	(0.486)	(5.282)	(4.656)	(0.485)	(0.389)	(0.412)	(0.502)	(0.360)	(0.473)	(0.154)	(0.173)
Israel	1,408	2.401	NA	9.438	9.515	0.887	0.560	35.863	30.893	0.250	0.169	0.413	0.703	0.171	0.315	0.575	0.560
		(1.282)		(0.753)	(0.808)	(0.316)	(0.498)	(5.652)	(5.771)	(0.433)	(0.376)	(0.493)	(0.458)	(0.377)	(0.466)	(0.167)	(0.155)
Italy	1,170	1.659	NA	9.100	9.262	0.910	0.629	37.763	34.651	0.284	0.164	0.134	0.269	0.143	0.152	0.437	0.432
		(0.666)		(0.599)	(0.422)	(0.287)	(0.484)	(4.856)	(5.753)	(0.451)	(0.371)	(0.341)	(0.444)	(0.350)	(0.360)	(0.134)	(0.137)
Luxembourg	666	1.714	NA	9.243	10.009	0.869	0.590	36.389	32.369	0.437	0.070	0.226	0.484	0.110	0.270	0.635	0.528
		(0.761)		(0.984)	(0.479)	(0.338)	(0.493)	(5.259)	(5.764)	(0.497)	(0.256)	(0.419)	(0.501)	(0.314)	(0.445)	(0.199)	(0.211)
Netherlands	1,606	1.931	NA	9.251	9.922	0.916	0.680	36.405	32.363	0.702	0.137	0.246	0.445	0.389	0.361	0.661	0.591
		(0.774)		(0.981)	(0.615)	(0.278)	(0.467)	(5.224)	(5.682)	(0.458)	(0.344)	(0.431)	(0.497)	(0.488)	(0.481)	(0.194)	(0.217)
Poland	6,419	1.842	NA	7.987	8.193	0.881	0.455	36.978	32.527	0.082	0.059	0.248	0.553	0.206	0.397	0.626	0.638
		(0.858)		(0.558)	(0.581)	(0.324)	(0.498)	(5.559)	(6.490)	(0.274)	(0.235)	(0.432)	(0.497)	(0.404)	(0.489)	(0.185)	(0.168)
Russia	820	1.581	NA	5.980	6.218	0.816	0.553	37.038	36.462	0.072	0.048	0.249	0.364	0.259	0.313	0.805	0.730
		(0.684)		(1.022)	(1.008)	(0.388)	(0.500)	(5.624)	(7.056)	(0.259)	(0.214)	(0.433)	(0.484)	(0.438)	(0.466)	(0.232)	(0.234)

(continued)

Table A1. (continued)

		No. of children		Natural log of annual earnings		Married/ Cohabiting		Age		Part-time status		Higher educational attainment		Professional/ Managerial worker		Proportion of women in occupation	
	N	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless	Mother	Childless
Slovak Republic	5,187	2.071	NA	7.257	7.441	0.904	0.484	36.337	36.189	0.037	0.008	0.114	0.207	0.125	0.203	0.644	0.620
		(0.822)		(0.520)	(0.421)	(0.295)	(0.500)	(5.452)	(6.190)	(0.189)	(0.088)	(0.318)	(0.405)	(0.331)	(0.402)	(0.192)	(0.213)
Spain	923	1.741	NA	8.792	8.990	0.924	0.790	36.656	30.978	0.191	0.115	0.266	0.362	0.195	0.222	0.574	0.541
		(0.754)		(1.051)	(0.836)	(0.265)	(0.408)	(5.212)	(4.632)	(0.393)	(0.320)	(0.442)	(0.481)	(0.397)	(0.416)	(0.148)	(0.146)
Sweden	3,606	1.985	NA	9.328	9.591	0.815	0.398	36.355	32.167	0.297	0.243	0.162	0.277	0.092	0.129	0.553	0.534
		(0.858)		(1.144)	(0.997)	(0.388)	(0.490)	(5.272)	(6.257)	(0.457)	(0.429)	(0.368)	(0.448)	(0.289)	(0.335)	(0.151)	(0.131)
UK	5,582	1.918	NA	9.410	10.150	0.833	0.732	36.867	33.089	0.512	0.083	0.146	0.343	0.164	0.316	0.585	0.555
		(0.819)		(0.917)	(0.620)	(0.373)	(0.443)	(5.264)	(5.993)	(0.500)	(0.276)	(0.353)	(0.475)	(0.371)	(0.465)	(0.171)	(0.182)
United States	13,544	1.990	NA	9.755	10.126	0.777	0.584	36.357	34.460	0.143	0.060	0.248	0.442	0.309	0.435	0.661	0.610
		(0.965)		(1.036)	(0.903)	(0.417)	(0.493)	(5.612)	(6.336)	(0.350)	(0.237)	(0.432)	(0.497)	(0.462)	(0.496)	(0.236)	(0.238)

Table A2. Country-Level Measures.

	% of 0 to 3 year olds in publicly supported childcare[a]	% of 3 to 6 year olds in publicly supported childcare[a]	No. of weeks of fully paid maternity leave[a]	No. of weeks of fully paid parental leave[a]	Maximum no. of weeks of job-protected leave[a]	No. of weeks of paternity leave[a]	Taxation of second earner's income (100% of APW)[b]	% Women employed[c]	% Workers employed in public sector[d]	Gini coefficient[e]	GDP per capita in current U.S. dollars[f]
Australia	13	41	0	0	52	0	32	65	16	.317	19,053
Austria	8	77	16	24	85	0	29	75	27	.257	24,194
Belgium	20	99	12	4	28	0.6	53	78	31	.279	22,623
Canada	5	53	8	6	25	0	36	76	19	.315	23,559
Czech Republic	1	76	19	32	162	0	30	76	22	.259	6,011
Finland	24	66	12	37	161	3	34	70	27	.246	23,543
France	22	99	13	53	159	0.6	26	72	30	.278	22,547
East Germany	34	87	14	13	161	0	53	78	23	.231	23,114
West Germany	5	75	14	13	161	0	53	67	22	.280	23,114
Hungary	10	88	24	73	159	0	30	71	37	.292	4,692
Ireland	4	56	10	0	14	0	31	62	18	.313	25,313
Israel	19	79	10	0	64	6	NA	61	17	.346	18,423

(continued)

Table A2. (continued)

	% of 0 to 3 year olds in publicly supported childcare[a]	% of 3 to 6 year olds in publicly supported childcare[a]	No. of weeks of fully paid maternity leave[a]	No. of weeks of fully paid parental leave[a]	Maximum no. of weeks of job-protected leave[a]	No. of weeks of paternity leave[a]	Taxation of second earner's income (100% of APW)[b]	% Women employed[c]	% Workers employed in public sector[d]	Gini coefficient[e]	GDP per capita in current U.S. dollars[f]
Italy	6	85	18	8	48	0	39	52	16	.333	19,269
Luxembourg	4	68	16	23	42	0.4	28	67	11	.260	46,277
Netherlands	6	68	16	0	16	0	40	76	25	.231	26,033
Poland	2	39	18	0	173	0	37	66	29	.507	6,620
Russia	21	64	20	12	165	0	NA	80	38	.434	1,775
Slovak Republic	9	78	25	36	161	0	NA	75	44	.189	2,215
Spain	5	77	16	0	161	0.4	23	52	26	.336	14,421
Sweden	41	86	7	50	64	2	34	86	34	.252	27,286
UK	1	71	8	0	18	0	24	69	19	.347	24,993
United States	6	53	0	0	12	0	30	73	16	.368	34,600

Note. APW = average production workers' wages; GDP = gross domestic product.
[a]Blinded.
[b]Jaumotte (2003b).
[c]Authors' calculations based on LIS data.
[d]International Labor Organization and authors' calculations based on LIS data.
[e]Luxembourg Income Study, key figures.
[f]OECD (2012).

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Notes

1. We discuss both the total, or gross, motherhood penalty and the residual, or net, penalty, meaning what remains after adjusting for the mechanisms known to produce the penalty. The gross penalty is important empirically and substantively because it reveals the full extent of earnings inequality associated with motherhood. The residual penalty may result from unobserved mechanisms, such as lowered productivity or employer discrimination.
2. While Mandel and Semyonov (2005) estimate supplementary models with separated measures for childcare and maternity leave, their article primarily focuses on a combined policy index. We extend the separate policy models approach begun in their work.
3. *Maternity leave* and *paternity leave* refer to birth-related leave typically accompanied by earnings-related benefits, while *parental leave* stands for longer leaves, typically job-protected with lower benefits or unpaid that enable parents to care for young children.
4. Including level and progressivity of income taxes, tax deductions for dependent spouses and children, joint or individual taxation of married couples, or income thresholds for social security contributions.
5. In many countries, cohabitation is akin to marriage. We include cohabiters as married couples.
6. We use Heckman regression models addressing the problem of estimating the effects of covariates on an outcome where respondents may be systematically selected into the group for which the outcome variable is observed. In the first step, we predict women's employment participation among all women using a dummy variable indicating the presence of a preschooler, household income excluding the woman's income, and nonfamily transfer income. The second stage of the Heckman model adjusts for each respondent's differential likelihood of employment in its estimation of how predictor variables in the main model are associated with earnings (Heckman, 1979).
7. Because Wave 5 data are not available or are of poorer quality, we use Wave 4 data for the Czech Republic and the Slovak Republic and Wave 6 data for Poland.

8. We use average annual exchange rates and consumer price index conversion factors (Sahr, 2001) to convert national currencies into U.S. dollars in year 2000. We also estimated models using earnings in national currencies, which had no effect on the fixed-effects coefficients. Because the conversion reduces the spread of the earnings distributions, the standard errors tend to be smaller in the models using logged 2,000 U.S. dollars. Finally, we also estimated models using within-country earnings percentile rankings (Mandel & Semyonov, 2005, 2006); this corrects for different levels of earnings dispersions across countries but did not change results.
9. Results were robust for the one-, two- and three-plus child dummy variables. While effects were not linear, they were monotonic, with higher numbers of children associated with larger wage penalties. Given the many interactions between number of children and country-level measures in our models, we simplify by using number of children rather than child dummy variables.
10. National survey data harmonized by LIS vary in the level of educational attainment detail. Following LIS, we used ISCED Levels 5 and 6 as an initial guideline to create the variable indicating high educational attainment but also used information on national education systems from each country to make decisions, particularly in cases where the national data do not map well onto the ISCED = 97 coding scheme.
11. While not an ideal measure of experience, this is commonly used when actual work experience is lacking (see Filer, 1993; Stewart, 2000).
12. In Finland, direct weekly hour measures are unavailable, though numbers of weeks worked full time and part time are available. If a respondent spent a majority of the weeks in part-time employment, he or she was coded as part-time employed. In the Slovak Republic and Poland, the part-time measure represents self-reported part-time status.
13. The measure of professional-managerial status is based on codes in the 1,000s and 2,000s in the International Standard Classification of Occupations ISCO-88 if available in the original survey. When these codes were unavailable, we derived as close an approximation as possible.
14. We examine former East and West Germany separately, due to the persistent differences in employment patterns and different policy legacies (Rosenfeld, Trappe, & Gornick, 2004).
15. Of course, it is likely that the lagged effect is longer, given our measurement of motherhood. Without longitudinal individual-level data, however, we believe this is the best approach to take.
16. Government policies related to the provision of childcare are hard to capture because policies for children of a specified age-group often do not correspond to the actual delivery of services due to financial or ideological barriers (Plantenga & Remery, 2009). We therefore follow standard practice in the literature and employ a measure of service usage. For a discussion of

the challenges of creating cross-nationally comparative childcare enrollment measures, see Eurostat (2004) or Rostgaard and Fridberg (1998).

17. ILO data were supplemented by authors' own calculations based on LIS data where ILO data were unavailable.
18. Nonfamily transfer income includes social insurance benefits (disability, sickness, unemployment), social assistance, or military or veteran's benefits, as well as regular cash transfers from family or relatives or charitable organizations. This measure excludes child-related benefits (child allowances, family benefits, maternity or parental leave benefits, alimony or child support). Because the reference period is the year prior to the survey, the receipt of unemployment benefits does not perfectly predict employment status in the survey week.
19. We use restricted maximum likelihood to estimate our models because restricted maximum likelihood provides less biased random-effects estimates than full maximum likelihood, especially in models with fewer Level-2 cases. The two methods produce the same fixed-effects estimates (Luke, 2004; Snijders & Bosker, 1999).
20. In contrast to random-slopes models, random-intercept models do not estimate the relationship between country-specific motherhood penalties and policies, but they recognize that individuals are embedded in different (country) contexts that may shape women's and mothers' employment decisions.
21. Exponentiated coefficients can be interpreted as the percent change in annual earnings associated with a one child increase in the number of children.
22. Because standardizing coefficients in multilevel models poses problems with regard to the standard deviations used to calculate Beta coefficients, particularly where cross-level interactions are involved, we provide only standardized coefficients for models including individual-level covariates only.
23. Because Sweden is an outlier on this policy measure, in results not shown, we top-coded Sweden to the next lower observed value and reestimated the models to ensure Sweden was not driving our findings. Results remained significant, in the same direction, and even slightly larger in size.
24. In results not shown, we included both paid maternity leave and fully funded parental care leave in the same model, results for both were robust.
25. Findings for the combination of maternity plus parental care leaves are equivalent to the results presented for parental care leaves alone.
26. In results not shown, we tried alternate specifications of labor force participation including mothers' employment rates, women's full-time employment rates, mothers' full-time employment rates, and women's and mothers' employment probabilities (generated by a logistic model using presence of a preschooler, education, age, and other household income and its square as predictors). Results were robust across all specifications, so we opted for the simplest specification (women's employment rates). We also ran models

excluding the IMR to check whether the presence of both women's employment rates (country-level measure) and the IMR (individual-level measure capturing selection into employment) impact the size of the cross-level interaction. Findings are robust with the exception of the interaction between childcare enrollment of 3 to 6 year olds and the motherhood penalty that is not significant.

27. Programs for 3 to 6 year olds vary in the daily number of hours and annual number of days children are in care. With a measure including the number of hours of care per year for 3 to 6 year olds, we might find stronger effects. Lewis (2009) provides the percentage of children in care (public or private) for 30 or more hours a week based on EU-SILC data, but for only a subset of the countries used in our analysis.

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